DISCUSSION BEFORE THE RADIO SECTION, 7TH MAY, 1947

Mr. J. A. Ratcliffe: In the series of experiments now under discussion the accuracy has been pushed so far that the magnitude of the measured velocity can be used to provide information about the medium through which, or over which, the waves travel. The velocity measured by Mr. Mendoza should refer to a "surface wave" and should be determined by the electrical properties of the ground. The fact that the velocity is significantly smaller than that measured by the other methods lends support to this view. We first note that it cannot be explained in terms of Zenneck's theory of plane surface waves, which leads to a phase velocity greater than that in free space. The proper explanation appears to be contained in a somewhat complicated mathematical paper by Russian authors* who have shown that, when a wave spreads over the ground from a point source, it travels for the first 20 wavelengths or so with a velocity less than that in free space, and at greater distances the velocity approaches asymptotically that in free space. At the Paris meeting of the U.R.S.I.† in September 1946, I drew attention to the fact that this somewhat surprising result is also implicit in the calculations published by Norton in 1941.‡ Very recently Norton§ has also pointed out that his 1941 calculations lead to interesting deductions about the way in which the velocity depends on the distance from the source.

Norton showed that the surface wave travels with the free-space velocity, but with a phase lag ϕ, which is a function of the "numerical distance" ρ as defined by Sommerfeld, and he gave curves of ϕ plotted against ρ for different frequencies and ground constants. The fact that ϕ varies with distance implies that the phase velocity is different from that in free space, and by differentiating ϕ with respect to (actual) distance, the velocity at any point is found. In this way it can be shown that the velocity is reduced over a distance of about 20 wavelengths, and if Norton's graph is used to compute the velocity for a special case which was worked out in the Russian paper, it is found to lead to the same result. Since the method of calculation from the Norton graph leads to the same result as that obtained by the Russians, the two theories are presumably equivalent, and, since I find the Norton method much the easier, I have used it in what follows.

In an attempt to see whether Mr. Mendoza's results can be explained by an approximate theory based on Norton's curves I have assumed, as a first approximation, that the measured velocity will be that corresponding to distances of the same order as the separation between the two transmitting stations. I have further assumed that the two stations radiate the same frequency (100 kc/s), and have sought for a value of the ground constants which will give rise to the experimental results. The appropriate electrical conductivity of the ground turns out to be 10^{-13} e.m.u., which appears reasonable for the wet ground over which the experiments were conducted.

A more accurate theory would take account of the different wavelengths emitted from the two stations, of the effect of the position of the points X and Y in which the course of the aeroplane intersects the extension of the base line, and of the height at which the observations are made. Mr. Elson of the Cavendish Laboratory is following up these points in detail.

It is of interest to note that the low velocity of waves over the ground is in the correct sense to explain coastal refraction, and it is probable that a theoretical explanation of this phenomenon is available in terms of the Norton surface wave. Grunberg* has proposed a theory of this kind. I have verified that a simple calculation of the above type, based on Norton's curves, gives the right order of magnitude to explain the original coastal refraction results of Eckersley, if we assume that the transmitter is a reasonable distance from the coast. This type of explanation is not, at first sight, satisfactory when the receiver, and not the transmitter, is near the coast, and there are obvious difficulties with the requirements of the reciprocity theorem. We are examining the matter in more detail.

Dr. H. G. Booker: It is interesting to notice from the experiments of Dr. F. E. Jones that, when the time of travel of a radio signal between two points near the earth's surface can be measured to an accuracy better than one part in 10^4, some variability in the time-delay can be detected. As Dr. Jones mentions, one would expect variability in meteorological conditions to affect the time-delay to a small extent, and that this variability might be just detectable with a navigational aid as accurate as the one he was using. That this variability should have been detected in an experiment carried out in Great Britain and lasting for a comparatively short period is a little remarkable. The weather of Great Britain, although variable, is not subject to extremes, if considered in relation to the world as a whole. There are parts of the world where far greater extremes of climate are experienced than in this country, and where, in consequence, appreciably greater variations could occur in radio propagation-time between two points close to the earth's surface. There should in fact be quite a close correlation between occurrence of abnormal time-delays and occurrence of atmospheric refraction. It is well known† that there are many parts of the world where super-refraction is much more intense and frequent than in Great Britain, and it must be expected that a similar result is true for meteorological changes of propagation-time. The fact that Dr. Jones seems to have been able to detect in this country variations in propagation time with meteorological conditions, suggests that, in some parts of the world, such variations may be of some practical significance for highly accurate navigational aids.

Dr. R. L. Smith-Rose: My interest in this particular subject was aroused in the early days of the 1939–45 War, when the question as to what was the velocity of light and how accurately it was known, arose in connection with various applications which were being developed at that time. It was of great importance to know to what extent, if any, the velocity of radio waves in the radio frequency part of the spectrum differed from the velocity of light, and we have seen in these papers a very satisfactory resolution of at least part of the problem.

I feel that in presenting their papers the authors have smoothed over what must have been a considerable number of difficulties. It is obvious to those who are familiar with radio technique that a great deal of thought must have been devoted to making measurements to the accuracy given, and I think the results are altogether satisfactory.

The velocity of light in air is, as Dr. Smith pointed out, 299 680 km/sec. As he explained, the results given in his paper and those in Dr. Jones's paper show that, for very short radio waves, the mean value of the velocity deduced is equal to the value for light waves, well within the accuracy of the measurements. Mr. Mendoza, on the other hand, stated that his measurements gave a value of 299 250 km/sec for the longer wavelength (of the order of 3 000 m) and for conditions, as he explained, of overland transmission. I should like to draw attention to the

* ALPERT, J. L., MIGULIN, V. V., and RJAZIN, P. A.: *Journal of Physics* (*U.S.S.R.*), 1939, 1, p. 381 (in English).
† RATCLIFFE, J. A.: Proceedings of the U.R.S.I. 7th General Assembly, Paris, 1946.
‡ NORTON, K. A.: *Proceedings of the Institute of Radio Engineers*, 1941, 29, p. 623.
§ NORTON, K. A.: *ibid.*, 1947, 35, p. 284.

* GRUNBERG, G.: *Journal of Physics* (*U.S.S.R.*), 1942, 6, p. 185; *Physical Review* 1943, 63, p. 185.
† BOOKER, H. G.: *Journal I.E.E.*, 1946, 93, Part IIIA, No. 1, p. 69.

fact that in the earlier work carried out some ten years ago in the U.S.S.R., using wavelengths of the order of 300 m, the mean result obtained was also 299 250 km/sec, although the accuracy claimed in those days (± 300 km/sec) was very much lower than that now given by Mr. Mendoza (± 40 km/sec). It is significant that while Mr. Mendoza gives an accuracy of his figures as ± 40 km/sec, the Russian work gave an accuracy of only ± 300 km/sec; even taking the lower accuracy, the difference between the values for light and long radio waves transmitted over land is greater than the order of accuracy. I suggest, therefore, that the confirmation by Mr. Mendoza, with the improved technique, of the earlier results obtained by the Russians is very gratifying; and it gives us great confidence to have from these two different sets of results confirmation of the fact that, for propagation over land and on medium or long waves, the land must play some part in the determination of the velocity. The point which at once comes to mind is whether we can repeat this measurement for conditions over the sea, and I hope that one day some investigator will obtain facilities for carrying out such an experiment.

The velocity of radio waves is also of interest, as Mr. Ratcliffe mentioned, in connection with coastal refraction in radio direction-finding, where we encounter a problem which has puzzled us for well over 20 years. We know quite well that medium-frequency waves, travelling from sea to land, are, on arrival at the coastline, deflected in a direction towards the normal to the coastline. There is also experimental evidence that it is not necessary for the transmitter to be near the point where the waves cross the boundary. What I am not so sure about is whether experimental proof can be given that both the transmitter and the receiver were remote from the boundary in cases where coastal refraction occurred. In certain cases the transmitter was certainly much more than 10 miles from the boundary, but in those cases the direction-finding system was only a mile or two from the boundary. That raises the question of whether the whole effect can be regarded as being of a reciprocal nature. I draw attention to the need for resolving this problem, because it is one which has not yet received a satisfactory explanation.

Mr. P. G. Redgment: I intend to confine most of my remarks to Mr. Mendoza's paper, but I should like to make two points with regard to the others. As has already been mentioned, the velocity of light may be expected to vary with meteorological conditions; anomalous-propagation effects show that the refractive index of the atmosphere does, in fact, vary in this way. It would be interesting to know, therefore, whether any experiments have been attempted to correlate meteorological conditions, velocity and observations of anomalous propagation. The Germans did some work on the ultimate accuracy of a v.h.f. H-system, including an estimation of variations in velocity due to the atmosphere. Their figure for the fluctuation to be expected was of the order of two parts in 10^4.

Returning to Mr. Mendoza's paper, I think a good deal more attention might have been paid to the causes of error in such a measurement. Experience with accurate measurements with this type of instrument has shown that there are a large number of possible sources of error, e.g. calibrations of the individual receivers, and a much more detailed examination of the limits of error could profitably have been made in the paper.

A rather more serious objection is that raised by Mr. Ratcliffe. He has pointed out that the effect of finite earth conductivity is not a constant alteration in velocity, but rather a change of phase which varies with distance; Mr. Mendoza has treated the effect as though the result was that the wave was propagated with a constant velocity lower than that of light. What matters in the Decca system is the *difference* between the phase changes experienced by the signals from the two transmitters. In point of fact, this phase difference changes only slowly with distance, with the average separations encountered in the Decca system, so that in this case the velocity can be considered as constant over a certain area. What is measured in Mr. Mendoza's experiment is an "effective velocity" for the substitution of the expression for calculating a pattern for the Decca system in a given area. It is not a genuine phase velocity, as the latter can only have a particular value at a particular point. A colleague of mine has started to calculate the errors in the Decca pattern due to the changes in phase velocity, and although the calculations are not yet complete, it does appear that the assumption of a constant velocity—as determined by Mr. Mendoza's experiment—enables a hyperbolic pattern to be calculated which fits very closely to the true pattern over a useful area; and this, of course, is the thing which matters to the engineer designing a navigational aid.

Mr. Ratcliffe has pointed out that, even without considering dispersion, change of phase is to be expected along the base-line extension. Mr. Mendoza's experiment is not of sufficient precision to determine whether or not the expected change occurred over the distance used. It may be that it is not possible to determine whether or not there is appreciable dispersion by the presence or absence of such change, since it is expected even without dispersion.

Coming to the question of phase or group velocity, I cannot feel that it is really relevant to the rest of the paper. The expression shows that the velocity measured depends upon both v_A and v_B. Since no method was available in this experiment to determine whether or not dispersion existed, it is impossible to distinguish between v_A and v_B, and the measured velocity is a combination of the two. The justification for treating this as a definite velocity is that v_A and v_B are sensibly the same; there is then no need to distinguish between group and phase velocities. If a difference is expected to exist between group and phase velocity, the experimental method is not available to enable either velocity to be determined.

I would suggest that Mr. Mendoza's paper describes a method of obtaining an "Effective Phase Velocity" which is useful in the design of certain navigational-aid systems, but it will be necessary to be very careful in giving any more general interpretation to the results.

Dr. L. Essen: I should like to make one or two comments on the errors involved in the measurement of the velocity of propagation of electromagnetic waves. In all cases they can, I think, be resolved into three groups, namely those depending on the measurement of a frequency, those depending on the measurement of a distance, and those—which may be called observational errors—depending on the association of a certain distance with a certain frequency. In the velocity of light measurements I think the first two of these can be made negligibly small, but the observational errors are quite large, and, in fact, wide variations are obtained between the results of individual determinations. To reduce this error, a great number of determinations are made—in some cases over 3 000—and although I would be the last to disparage the great amount of skill and patience that has been devoted to these measurements, I think a result based on a statistical mean is rather unsatisfactory. There is good reason therefore to explore other ways of measuring the constant, and the methods described may be important from this aspect as well as from those discussed by other speakers. The limit of accuracy in the case of the most precise of these methods described by Dr. Jones appears to be set by the observational errors, and the probable error might therefore be reduced if a greater number of observations were taken. In considering his deviation of ± 25 km/s it should be remembered that the

deviations of ± 11 km/s and ± 14 km/s in the velocity of light results, quoted by Messrs. Smith, Franklin and Whiting, are the deviations of the means of groups of determinations and not of individual determinations. There is still the difficulty of correcting for the refractive index of the medium, but if samples were tested for humidity the correction could probably be made with sufficient accuracy.

The recently-developed techniques employing centimetric waves have made possible other methods of measuring the velocity of propagation. One that I have recently used employs the resonance of a length of waveguide. The promising feature of this method is that the observational error is small—less than one part in 10^5—and the limit of accuracy is set rather by imperfections in the waveguide and by the effects of coupling to the oscillator and detector. These effects can be investigated both theoretically and experimentally, and it may be possible to reduce the total error to less than one part in 10^5. The result so far obtained for the velocity in a vacuum* is 299 793 km/s, the estimated maximum error being ± 9 km/s. I do not know whether it is of significance in view of their limited accuracy, but the results we have heard to-night are also a little higher than those deduced from the velocity of light.

Mr. N. Elson: Working under Mr. Ratcliffe at Cambridge, I have developed in considerable detail his suggestion of interpreting Norton's phase correction in terms of the variation of velocity of radio waves with distance. I have also looked into its application to the problem of coastal refraction and have shown that Eckersley's measurements can be accounted for satisfactorily. Mr. Ratcliffe's second suggestion of the effect of this variation in velocity with distance on navigational systems has also been investigated in detail. In particular, I have worked out complete error diagrams for the Decca Navigator system. For short distances they are fairly simple diagrams and have the same sort of shape whatever pair of stations is used. At greater distances, however, a certain asymmetry appears in the error contour pattern, and depends upon the two particular frequencies used for the master and slave stations. I shall restrict my remarks to such calculations affecting Mr. Mendoza's measurements.

Turning to the general case of the determination of velocity as a function of distance, there are four velocities of interest, namely (*a*) the instantaneous phase velocity; (*b*) the mean phase velocity for the path from the transmitter to the receiving point (that commonly measured); (*c*) the group velocities corresponding to (*a*) and (*b*), of interest for measurements such as Dr. Smith's, where the actual transit times of pulses are measured. I have derived formal expressions for these four velocities in terms of the Norton phase correction ϕ and its derivatives with respect to distance, i.e. with respect to the Somerfeld numerical distance. With the group velocities, the dependence of the numerical distance on frequency naturally occurs. For wavelengths above about 50 m, the conduction current over land is considerably greater than the displacement current, so that the numerical distance depends quite simply on the frequency used, and therefore one can easily work out how those velocities vary with distance. All the velocities, with one exception, start lower than the velocity of radio waves in free space and gradually increase with increasing distance, until at large distances they are equal to the velocity in free space. The exception is the instantaneous group velocity, which starts below the velocity in free space and increases until it becomes equal to the free space velocity at a numerical distance of about 1 or 2; it then rises above the free-space velocity, passes through a maximum, and then decreases again to the free-space velocity.

The suggestion that the group velocity may be greater than the velocity of light may raise certain objections. These can, however, be met, as it is only the instantaneous group velocity and not the mean group velocity. For wavelengths over 50 m, assuming that the land has a constant conductivity, the numerical distance is proportional to the square of the frequency used. The numerical distances over which Mr. Mendoza made his measurements were comparatively small, and it is possible to derive a fairly simple approximation from Norton's curves for this particular case. In those circumstances, the Norton phase correction is proportional to the square root of the numerical distance and therefore proportional to the frequency. The total phase is that calculated on the assumption of free-space velocity plus the Norton correction, and, since both the terms are proportional to the frequency, the group and phase velocities are identical. Hence, for small numerical distances at least, the group and phase velocities both vary identically with distance. At larger numerical distances, a slight difference between the phase and group velocities becomes apparent, but only at the stage when both velocities have become so close to the velocity in free space that the difference—with our present precision of measurement—is unimportant. Thus the phase velocity measured by Mr. Mendoza would equal the group velocity under the same conditions and may therefore be compared with the measurement of group velocity made by Dr. Smith and his associates. It is easy to show that for Dr. Smith's experiment the velocity is effectively the velocity in free space.

Because Mr. Mendoza used short distances and long wavelengths, the Norton corrections are very important. By more detailed calculations, I have confirmed that Mr. Ratcliffe's figure of a conductivity of 10^{-13} e.m.u. is sufficient to account for the difference between the two sets of measurements. Furthermore, I have calculated what would be expected in phase differences for aircraft circuits crossing close to the stations and then far away from the stations. The effect of the Norton correction means that the total phase change going from a point on the one base-line extension to the other base-line extension is increased, and has a maximum increase when the crossing points are close to the stations. In the author's experiment the crossing points varied from about 10 to 50 or 60 miles from the stations. Taking two circuits, say one at 10 miles and the other at 40 miles, there should have been a difference in the measured phase differences of about 5–7 div on the Deccometer dials; the author's accuracy is about 2 div. The author has very kindly given me his detailed results, from which I find that the differences for four sets of measurements are: (1) 4–5 div; (2) one div; (3) 3–4 div; (4) 6 div. Unfortunately, the effect is in the opposite direction to the theory, the smaller phase differences occurring close in and not far out, as one would have expected. No satisfactory explanation has been found yet.

Dr. E. H. Rayner: I think it will be interesting to give some results of experiments which were conducted a good many years ago by the National Physical Laboratory, who, in collaboration with the Union Radio Scientific International (U.R.S.I.), undertook to organize the emission of a standard modulation frequency so that laboratories in Europe could calibrate their frequency standards. In those days few laboratories were equipped with crystal oscillators of high stability; tuning forks were more common.

My colleague Dr. Essen was in charge of the work at the N.P.L. A 1 000-c/s signal was transmitted over a telephone circuit to the B.B.C., who modulated one or more transmitters by it. This frequency was derived from a quartz-ring oscillator of 20 000 c/s, having a stability of the order of one part in 10^8. The work was carried out in the early hours of the morning, and the emission was continuous for an hour and a half, being preceded and followed by shorter periods of a transmission on a

* ESSEN, L.: *Nature*, 1947, 159, p. 611.

frequency differing by about one part in 10^6 from that of the main frequency in order that observers might determine whether their local frequency was higher or lower than the main frequency.

For the last of these experiments (March 13, 1935), the B.B.C. put three stations at our disposal, namely Scottish National (1 050 kc/s), Scottish Regional (464 kc/s), and Droitwich (200 kc/s). For the main emission these were modulated in parallel for the period from 01.30 to 03.00 G.M.T.

One of the results was that the modulation frequency on arrival tended to be unsteady in a manner which was unexpected. Short-time unsteadiness would be of a type associated with fading. However, it was reported from Riga, for instance, by Dr. Walter, that the modulation frequency on arrival might differ slightly but persistently from its correct value, in one direction for periods of the order of half a minute to one minute. The effect is similar to that of a steady alteration in the length of the wave path, and is more pronounced at the higher carrier frequencies.

ease with which frequencies or their derivatives can be compared with very high precision, provide a research tool for the physical qualities of media involved in radio transmission, the scope of which has been enlarged by the developments of the last 12 years. The work described in these papers is a notable example of progress of this kind.

Capt. C. F. Booth: I should like to refer briefly to the point raised in the discussion by Dr. Rayner, regarding standard-frequency transmissions. Although the subject he has raised is, perhaps, not of immediate concern to the work described by the authors, it is desirable to indicate the results of more recent work. Recently precise measurements have been carried out in this country on the standard-frequency transmissions from the U.S.A. by the Bureau of Standards Station WWV. This series of transmissions is extremely stable in frequency, being derived from a high-grade crystal-controlled oscillator of the type used in primary frequency standards, and the variation of the drive

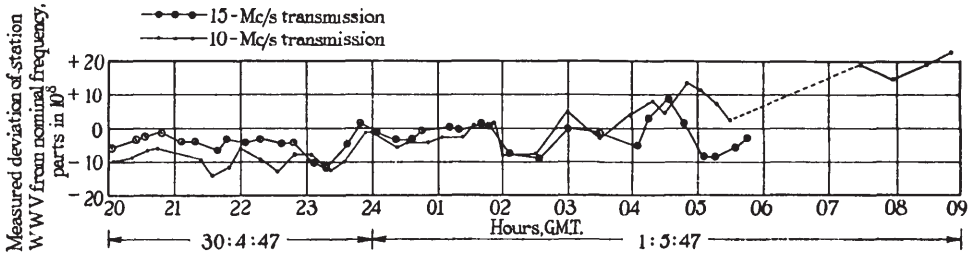

Fig. A.

Dr. Walter estimated that, using the Droitwich station, not less than 2 min would be required to attain an accuracy of one part in 10^6, and 20 min for an accuracy of one part in 10^7.

Unsteadiness of this or a similar type is measurable, however, over a considerable shorter distance on the long-wavelength transmissions from Droitwich. The late Dr. Braillard, Director of the International Control Centre at Brussels, telegraphed that the modulation frequency as received was slightly unsteady and asked us to look for a possible cause. Such a cause might have been at the N.P.L. or on the line circuit through London to Droitwich. Experiment showed that these were satisfactory, and a later emission of a similar modulation frequency in daylight (09.00–09.30, 27th March) indicated that the effect was largely due to night conditions, an unsteadiness of one part in 10^6 at night being reduced to one part in 10^7 during the day.

The frequency stability of modern crystal equipment, and the frequency about the allocated value is understood to be within $\pm 2 \times 10^{-8}$. The recent measurements, made simultaneously on the 10- and 15-Mc/s transmissions with an accuracy of $\pm 1 \times 10^{-8}$, have shown that the measured frequencies of the two transmissions in this country vary up to 50×10^{-8} from the nominal values. They also show that the measured variations on the two transmissions (which are derived from a common source, and hence at the transmitter possess the same percentage accuracy), do not correspond simultaneously on the two transmissions. The effect is doubtless due to changes in the path length of the received signals caused by drift of the appropriate ionospheric reflecting layers. The actual results of over-night measurements on the 10- and 15-Mc/s transmissions are shown in Fig. A. Each plotted point represents the average of a number of readings, generally three, taken over a period of 30–60 sec.